Alytes, 1997, **15** (3): 133-136. *Book review*

An evolutionary biologist's view on the science of biology

Alain Dubois

Laboratoire des Reptiles et Amphibiens,
Muséum national d'Histoire naturelle,
25 rue Cuvier, 75005 Paris, France

Mayr, Ernst. – ***This is biology. The science of the living world.*** Cambridge, Massachusetts & London, England, The Belknap Press of Harvard University Press, 1997: i-xvii + 1-327.

Born in 1904, Ernst Mayr is now a living legend of evolutionary biology His countless original contributions to the development of this discipline were published in about 650 journal articles and about twenty major books, starting with *Systematics and the origin of species* (1942), and going through landmarks such as *Animal species and evolution* (1963), *Principles of systematic zoology* (1969) or *The growth of biological thought* (1982). Ernst Mayr's last book, *This is biology* (1997), his last brilliant contribution to the understanding of the unique characteristics of biology among the sciences and of its particular philosophical bases, is of particular importance for all biologists. It provides a detailed analysis of several major questions often put by biologists, by scientists of other disciplines, and by lay persons, about the science of the living world.

Much has been written about the history and epistemology of biology, but many of the authors of these works were historians of science or philosophers, not biologists. The interest and significance of Mayr's new book come mostly from its having been written by a biologist, who has an inside understanding of the problems of this scientific field. As is very aptly shown by Mayr in this book, the philosophy and history of science have long been dominated by a "conception of science" derived from *physics*, and such a conception cannot address many aspects of the science of life. The characteristics of biology are very different from those of physical sciences and other sciences of matter, and generalisations drawn from the study of the latter often do not apply to biology, because of the unique particularities of life among natural phenomena.

Among the important differences, physics and other matter sciences try and draw *laws* having a general, universal value. A long-prevailing conception has also tried to assign this aim to biology. Such a conception can be illustrated by sentences such as· "science only deals with general matters" or "if you explain the bacterium to me, I'll leave you quits with the elephant or man". In such a perspective, it would be enough to study in every detail a single kind of organisms (a bacterium, a fruit fly, a white mouse) to know everything about life. However, this reductionist perspective has severe limits.

For sure, a number of particularities are common to all or most organisms, such as the genetic code, the basic cellular biochemical and physiological processes, or certain characters common to members of major clades. But, beside these common features, organisms are more correctly characterized by their *diversity* and the diversity of their characters. Contrary to what the reductionist approach suggests, this diversity is not a secondary phenomenon, or a disturbing "noise" in the study of life, but it is the main characteristic of life. This diversity is the result of the *evolution* of organisms on our planet, which has involved two distinct mechanisms: a progressive *modification* of the characteristics of organisms within evolutionary lines (process of *anagenesis*), and a *multiplication* of the clades of organisms (process of *cladogenesis*) both mechanisms together are traditionally known as the process of *phylogenesis* While the terms phylogenesis, anagenesis and cladogenesis refer to *processes*, their *results* may be known together as *phylogeny*, and I further suggest that the components of the latter could be recognized separately as *anageny* and *cladogeny*. As a consequence, biology is characterized much less by the existence of *laws* than by the fact that most of the characteristics of organisms can be explained because these organisms are the result of a *history*. Although this is not fully understood by many biologists, much more than a *deterministic* science, biology is a *historical* science.

Another important particularity of biology, well underlined in MAYR's book, is that nothing makes sense in biology if one does not understand that biological phenomena have *several distinct levels of integration*. This means that the properties of an organism cannot be reduced to those of its cells or organs, those of a population to those of its individuals, etc. At each level of integration, new particularities emerge, wich cannot be mechanically deduced from the particularities present at the immediately lower integrative level. This very old notion can be summarized in a sentence like: "the whole is more than the sum of its parts". It is qualified by Ernst MAYR, after others, under the term of *emergence*. Consideration of this characteristic of life is of paramount importance to understand many biological phenomena, and it supports a holistic, rather than a mechanistic or reductionist, approach to these phenomena.

Another strong quality of MAYR's book is to show in detail how, in science, several types of questions can be legitimately asked, and how these questions can be reduced to three major categories: the questions "what", "how" and "why". Many scientists, and even biologists, tend to believe that the only "legitimate" scientific questions are "how" questions: how does such biochemical or physiological mechanism work, how does things work inside the "black box" of such behaviour, etc. Such questions can often be studied through an experimental approach, and, to this day, some biologists still tend to think that scientific knowledge can be obtained only through experimental method. This is easy to understand in countries, such as France, where biology has long been a mere subdiscipline of medicine, and where great historical figures in research are those of people, such as Claude BERNARD or Louis PASTEUR, who were wearing white gowns and devoted most of their life to laboratory experimental studies. The situation is different in Anglo-Saxon countries, where there exists a strong tradition of field naturalists, such as DARWIN or MAYR, which has facilitated the emergence of a different, more holistic, approach to biology.

As a matter of fact, it is totally incorrect to state that the question "how" is the only legitimate question in science. This question allows to elucidate only one type of problems in biology, those which can be designated under the term "proximal causes". Thus the experimental method allows to answer a question such as "what is the environmental factor that determines the fact that an animal starts a seasonal migration?", but not the question "why did seasonal migrations appear in this species?". Such "why" questions aim at elucidating "evolutionary causes" of biological phenomena. They are as legitimate as "how" questions, because in fact each biological phenomenon is the result of *two distinct causation systems*: evolutionary and proximal causes. More and more biologists have become aware of this double causation system and recognize the legitimacy of "why" questions in science. Usually such questions cannot be answered to through the experimental method. They are the major questions which "evolutionary biology" asks. For a long time, the only answers to such questions were theological ones, but since 1859 and the development of the concept of *natural selection* it has been possible to provide scientific answers for them.

But there is a third kind of questions in science: these are "what" questions. Questions of this kind are the first ones man asks in front of the world: what exists, what are the major characteristics of what exists, etc. The answer to such questions requires to have recourse to observation, description, comparison or inventory. Contrary to an ideology currently dominant in science, such questions are legitimate scientific questions. As long as no correct answer has been provided to them, the questions "how" and "why" are meaningless, or at least cannot be correctly set. "What" questions must therefore be respected and this also applies to those who study them. As a matter of fact, the dominant activity of scientists in all scientific fields (including the most "modern" ones) are of this kind. Careful and complete descriptions and inventories are the first steps that cannot be done without in all fields, including molecular biology (description and inventory of molecules and of their activities), genetics or ecology.

An important proportion of "what" questions in biology is the set of questions that relate to the inventory and classification of biodiversity. Given the importance of these questions in the previous works of Ernst MAYR, it is not surprising that he devoted a nice chapter to this topic in his new book. Once again, MAYR comes back in this book to the many problems related to the building of biological classifications, which he had already discussed in several other important works (MAYR, 1969, 1974, 1981; MAYR & ASHLOCK, 1993). He very persuasively shows how the different "schools" of macrotaxonomy, and in particular the two dominant ones in the recent years (cladistics and evolutionary or "Darwinian" classification) do in fact differ in their objectives. The aim of an evolutionary *classification* is to provide a *taxonomy*, i.e. a hierarchical arrangement of *taxa*. The latter are *classes* that should be recognized on the bases of two criteria: genealogy (common descent) and degree of similarity (amount of evolutionary change). These two criteria correspond to the two dimensions of phylogeny: anageny and cladogeny. MAYR quite rightly suggests that arrangements of organisms built on the basis of cladogeny alone do not deserve the qualification of true classifications, but should rather be known as *cladifications*. The latter recognize units which are not classes (as are taxa), but clades, renamed by MAYR (1995) as *cladons*. To complete MAYR's terminological clarification, some other terms can be useful: the term *phylon*, first proposed by DUBOIS (1991) to designate the concept later called cladon by MAYR (1995), would be more appropriate to designate a phylogenetic s.l., i.e. both anagenetic and cladogenetic, unit. Following MAYR's conception of classification, the term phylon is therefore a strict synonym of the term taxon. While classification according to phylogeny s.l., i.e. both anageny and cladogeny, gives birth to what can be called either a *taxonomy* or a *phylonomy*, cladification, i.e. classification based on cladogeny alone, results in a *cladonomy*, not properly a taxonomy. The differences between the two systems can also be stressed when one considers the way characters are used to define taxa. In a traditional taxonomy, a taxon can validly be *diagnosed*, i.e. characterized by a *diagnosis* (or *taxognosis*, or *phylognosis*): this is a set of *differential* or *diagnostic* characters, both plesiomorphic and apomorphic ones, that characterize this taxon and distinguish it from related ones. On the other hand, in a cladonomy a cladon needs only be *apognosed*, i.e. characterized by an *apognosis* (or *cladognosis*, or more shortly *clagnosis*): the latter only includes *apognostic* characters, i.e. autapomorphic characters of the cladon, not shared with closely related ones.

Such terminological discussions may appear gratuitous or superfluous to some, but they are not. As shown on several occasions in MAYR's book, during the whole history of biology, many scientific debates, discussions and conflicts turned out to be ultimately caused by terminological confusions. Many so-called disagreements between colleagues simply take their root in the fact that these different biologists used the same term in different senses. Introduction of the term cladification is therefore a particularly useful contribution, which hopefully will be followed by all evolutionary biologists and systematists. It will help more and more people to understand that both classification and cladification may be legitimate, but that they do not have the same objectives. Evolutionary (or Darwinian, or synthetic) classifications serve multiple purposes, both practical and theoretical: their aim is to provide a hierarchical arrangement of taxa, the latter being non-polyphyletic and homogeneous groups of populations or taxa, about which the highest possible number of generalisations and predictions can be made. On the other hand, the aim of a Hennigian cladification is merely to give a transcription, under the form of a hierarchical arrangement, of a cladogram, and therefore to provide information on the branching pattern of clades in the phylogeny of a group. Both aims may be justified, depending on the information one wants to obtain, but it is important not to confuse both kinds of information storing systems.

Another clarification is wanting in MAYR's new book, as in his previous texts: MAYR rightly stressed on several occasions that the term *monophyletic* was used by cladists for a concept quite different from that designated by HAECKEL (1868) when he coined this term. As a result, in this book MAYR once again claims that the term monophyletic (and its derivative monophyly) should be restored in their original senses. Given the number of recent publications where these terms were used in HENNIG's (1950) new sense, I do not think this restoration will ever take place for all biologists, and, in my opinion, in order to avoid the continuation of confusion, these terms should be abandoned altogether. For "monophyletic sensu HENNIG" (i.e., a qualification of a group which is both non-polyphyletic and non-paraphyletic), ASHLOCK's (1971) term *holophyletic* should be used. For "monophyletic sensu HAECKEL" (non-polyphyletic), DUBOIS's (1986) term *homophyletic* is available. My feeling is that in this case one should follow the same line of reasoning as that advocated by MAYR & ASHLOCK (1991: 276-277) regarding the case of the terms *character*, *character state*, *signifer* and *signifer state*: "We realize that the character-character state terminology has been too widely adopted to be easily dislodged. Therefore, any endeavor to restore the traditional meaning of the word *character* would cause considerable confusion. Hence, although with considerable reluctance, we use *character state* for what traditionally has been called a character." A similar "considerable confusion" might arise from a continuous effort to restore the traditional meaning of the term monophyletic, and much clarification would come from a rejection of this term and its replacement by either holophyletic or homophyletic according to the purpose.

MAYR's book is still rich of many other stimulating discussions and analyses. After "what" questions, case studies of "how" and "why" questions allow to illustrate other important epistemological aspects of biology as a science with no equivalent among other scientific fields. Two chapters are devoted to the kind of questions that ecology asks, and to the status of our knowledge about human evolution. Possibly the least convincing chapter is the last one, which deals with the relationship between our knowledge of biological facts (and mostly evolution) and ethics. This chapter could have benefited from consideration of other works published on this matter by authors not mentioned by MAYR, such as the numerous books of Jean ROSTAND, to give only one example. Strangely, regarding human phenomena, MAYR seems to share the conventional reductionist attitude of many other biologists, who think that most psychological and social human features can be explained in terms of biology – if not simply of "common sense" (see e.g. pp. 39-40, 254, 260, 264-265, 267). However, here also, *emergence* is at work. It is as misleading to analyse these high-integration-level phenomena with biological concepts as to analyse biological phenomena with concepts from biochemistry or physics. During the last century, all scientific disciplines dealing with man (psychology, social sciences, economics, etc.) showed a great development and experienced several "scientific revolutions" as significant as those of GALILEO and DARWIN in their respective fields. Unless one is ready to accept that man, being "special among God's creations", cannot be studied scientifically, these developments must be duly considered. In order to adopt a scientific attitude in this respect, there is the same need of terminological and conceptual clarity and rigour as that aptly advocated by MAYR himself regarding biology, and, because scientists are humans, "common sense" is even more misleading here as it is in biology. Ignorance by MAYR of most of the significant works and theories concerning human behaviour, psychology and society severely limits the interest and reach of this chapter of his book.

No one now knows what the biology of the next century will be. Concerning the part of this science that deals with the diversity of life (systematics), which has attracted most of the attention of MAYR in his works, some authors (e.g.: CROWSON, 1970; DUBOIS, 1988) have suggested that "experimental systematics", dealing in particular with developmental problems, might be the next important step in this old research field. Whatever the case may be, terminological and conceptual clarifications will be of paramount importance for future progress of the discipline, and MAYR's works will have provided a lot of new and useful elements in this respect.

LITERATURE CITED

ASHLOCK, P. D., 1971. – Monophyly and associated terms. *Syst. Zool.*, **20**: 63-69.

CROWSON, R. A., 1970. – *Classification and biology*. London, Heinemann: i-ix + 1-350.

DUBOIS, A., 1986. – A propos de l'emploi controversé du terme "monophylétique": nouvelles propositions. *Bull. Soc. linn. Lyon*, **55**: 248-254.

----- 1988. – The genus in zoology: a contribution to the theory of evolutionary systematics. *Mém. Mus. natn. Hist. nat.*, (A), **140**: 1-123.

----- 1991. – Nomenclature of parthenogenetic, gynogenetic and "hybridogenetic" vertebrate taxons: new proposals. *Alytes*, **8**: 61-74.

HAECKEL, E., 1868. – *Natürliche Schöpfungsgeschichte*. Berlin, Reimer: i-xvi + 1-568 + 204 b-c, pl. 1-8 + 1.

HENNIG, W., 1950. – *Grundzüge einer Theorie der phylogenetischen Systematik*. Berlin, Deutscher Zentralverlag: 1-370.

MAYR, E., 1974. – Cladistic analysis or cladistic classification? *Z. zool. Syst. Evol.-Forsch.*, **12**: 94-128.

----- 1981. – Biological classification: toward a synthesis of opposing methodologies. *Science*, **214**: 510-516.

----- 1982. – *The growth of biological thought. Diversity, evolution, and inheritance*. Cambridge, Mass. & London, Belknap Press of Harvard Univ. Press: i-xiii + 1-974.

----- 1995. – Systems of ordering data. *Biol. & Phil.*, **10** (4): 419-434.

MAYR, E. & ASHLOCK, P., 1991. – *Principles of systematic zoology*. Second edition. New York, McGraw-Hill: i-xx + 1-475.